

**A measurement strategy to monitor and
assess changes in the Sierra Nevada water
balance using satellite and ground
observations**

Roger C Bales

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0239: A measurement strategy to monitor and assess changes in the Sierra Nevada water balance using satellite and ground observations

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Summary: The project addresses an important issue of better quantifying the Sierra Nevada water balance to improve water resource decisions. The investigators propose a standard format of integrated field measurements, remote sensing, and modeling, but make use of novel methods (sensor web network), and will produce near-real-time data which should be valuable for decision makers. Primary criticisms identified by the technical reviewers concern site selection (representative?) and method for delivering near-real-time data to decision makers and operations managers. Technical reviewers agreed on their overall rating of the proposal as very good, although Review 2 was non-critical without much substance. However, this proposal received a rating of adequate from the primary panel reviewer for the following reasons: 1) numerous scaling issues were not addressed for network design and data interpretation; 2) it was unclear what the observation density would be and how the new observation sites would be distributed across different land types; 3) time of year that sites would be visited was unclear (spring is the most relevant time for forecasting applications); and 4) model and observation errors were not sufficiently discussed. Goals: Clear, consistent and timely. Project goals are to better measure, model, and manage Sierra Nevada water balance using

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an integrated measurement and modeling approach that will provide near-real-time data. Important and timely topic given concerns regarding climate change and the importance of snow budgets for driving water resources for ecologic processes and social/economic use in California and western North America. Justification: The study is well justified in terms of the need for improving water forecasting and efficient management. Supported by detailed conceptual model. Model not new, but this is not a detriment. Could have been better/more explicitly linked to CALFED needs/objectives. Selection of research justified as a pilot study prior to more comprehensive work. One reviewer requested further demonstration that the current observational network is insufficient. Approach: Approach is generally well designed, using a standard format of integrated measurements and modeling. The sensor web network is novel and the focus on complex mountainous terrain and production of near-real-time data will be valuable to decision makers. Location of measurement sites as generally representative of the larger physiography/landscape is questioned. Additional data collection suggested by Reviewer 3, but not an impediment to the proposed work. However, it was unclear what the observation density was or how new observation sites would be distributed, or nested, among different land cover types. Moreover, the authors do not identify the time of year that site visits would occur (winter vs. spring). The latter is most relevant for flow forecasting. Finally, little detail is provided regarding how the authors will account for errors in both models and observed data. Feasibility: Approach could have been more fully documented. Technically feasible and high probability of success given PIs' experience, capabilities, and study design. Monitoring: Not applicable. Products: Primary product of near-real-time data will be highly valuable for decision makers, but method of delivery, access, and management of this data is vague. Also, better statement is needed of "who (specifically) will benefit" from these data. Peer-review publications are expected. Capabilities: The PIs are highly qualified and have considerable prior experience with this type of work. Budget: The budget is high for a pilot project. In particular, Reviewer 1 questions the high cost of the JPL sensor web and requests details/justification.

Additional Comments:

Summary: The project addresses an important issue of better quantifying the Sierra Nevada water balance to improve water resource decisions. The investigators propose a standard format of integrated field measurements, remote sensing, and modeling, but make use of novel methods (sensor web network), and will produce near-real-time data which should be valuable for decision makers. Primary criticisms identified by the technical reviewers concern site selection (representative?) and method for delivering near-real-time data to decision makers and operations managers. Technical reviewers agreed on their overall rating of the proposal as very good, although Review 2 was non-critical without much substance. However, this proposal received a rating of adequate from the primary panel reviewer for the following reasons: 1) numerous scaling issues were not addressed for network design and data interpretation; 2) it was unclear what the observation density would be and how the new observation sites would be distributed across different land types; 3) time of year that sites would be visited was unclear (spring is the most relevant time for forecasting applications); and 4) model and observation errors were not sufficiently discussed. Goals: Clear, consistent and timely. Project goals are to better measure, model, and manage Sierra Nevada water balance using an integrated measurement and modeling approach that will provide near-real-time data. Important and timely topic given concerns regarding climate change and the importance of snow budgets for driving water resources for ecologic processes and social/economic use in California and western North America. Justification: The study is well justified in terms of the need for improving water forecasting and efficient management. Supported by detailed conceptual model. Model not new, but this is not a detriment. Could have been better/more explicitly linked to CALFED needs/objectives. Selection of research justified as a pilot study prior to more comprehensive work. One reviewer requested further demonstration that the current observational network is insufficient. Approach: Approach is generally well designed, using a standard format of integrated measurements and

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modeling. The sensor web network is novel and the focus on complex mountainous terrain and production of near-real-time data will be valuable to decision makers. Location of measurement sites as generally representative of the larger physiography/landscape is questioned. Additional data collection suggested by Reviewer 3, but not an impediment to the proposed work. However, it was unclear what the observation density was or how new observation sites would be distributed, or nested, among different land cover types. Moreover, the authors do not identify the time of year that site visits would occur (winter vs. spring). The latter is most relevant for flow forecasting. Finally, little detail is provided regarding how the authors will account for errors in both models and observed data. Feasibility: Approach could have been more fully documented. Technically feasible and high probability of success given PIs' experience, capabilities, and study design. Monitoring: Not applicable. Products: Primary product of near-real-time data will be highly valuable for decision makers, but method of delivery, access, and management of this data is vague. Also, better statement is needed of "who (specifically) will benefit" from these data. Peer-review publications are expected. Capabilities: The PIs are highly qualified and have considerable prior experience with this type of work. Budget: The budget is high for a pilot project. In particular, Reviewer 1 questions the high cost of the JPL sensor web and requests details/justification.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The panel noted that this proposal was a pilot project to demonstrate the validity of improved snow monitoring and they believed this was an appropriate approach. However, it was considered to be an expensive "pilot". The project makes use of, and expands upon, existing data - the current data collection network is deemed to be inadequate. The panel felt that there were numerous scaling issues that the authors failed to address, both in terms of network design and data interpretation. Also, it was not clear what the observation density was or how new observation sites would be distributed,

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or nested, among different land cover types. The authors did not identify when (winter versus spring) site visits would occur and this could have large impact on the value and meaning of their field work (especially for stream flow forecasting applications) - to be most useful, field work should be conducted during the spring. The authors fail to address how they will estimate or account for errors in both models and observed data (from stations and satellites), and how information on model and observation error will be used to assimilate snow information into hydrologic models. Several additional steps are necessary before the proposal will be of use to decision makers.

Ranking: Adequate

Technical Review #1

proposal title: A measurement strategy to monitor and assess changes in the Sierra Nevada water balance using satellite and ground observations

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	All of the goals, objectives, and hypotheses are clearly stated and internally consistent. The ideas presented are timely and important, no doubt in my mind. Understanding, through measurement and modeling across spatial and temporal scales, hydrological issues associated with snowmelt in California's Sierra Nevada Basin, is an important topic to study now. The proposal is rich in discussion and supporting information describing the importance of the research topic, but did lack in specific details that are important to know in order to asses how the objectives will be met, and how the hypotheses will be tested. Overall, I would rank this proposal as "Excellent" in the Goals criteria.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	Yes, this study is justified relative to the existing knowledge. The PI has extensive experience and knowledge in the topical area, and does an excellent job supporting such justification through a detailed
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	<p>conceptual description of the need for such study. Overall, I think that the justification description (written almost like "white-paper"), although excellent, was at the expense of missing details in the methodology. A conceptual model was clearly stated, and it did explain the underlying basis for the proposed work. My only criticism here is that the conceptual model was nothing new to the discipline, other than the inclusion of the sensor pods (discussed below). The selection of the research project and full-scale implementation is justified. It is clear that the PI has spent considerable effort thinking about the research sites. This is partially explained by the fact that this study fits within the larger-scale research agenda of the SNHO (Sierra Nevada Hydrological Observatory). I agree with the PI that this project, if supported, would compliment rather than conflict with other potential projects. The full-scale implementation of the project, namely integration of measurements across several spatial scales with models to fill in the gaps, is the standard approach when budgets allow. The justification for such a proposal, based primarily on the need for a better understanding of snow-driven water resources in a topographically-complex region experiencing environmental changes (population growth and climate change), ranks this proposal as "Excellent" in this category.</p>
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The general approach is to integrate measurements across various spatial scales from point-measurements to remote sensing, and use various models to extend
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	<p>the results in time and space. This is not a new approach, and has been used by several studies supported by resources of the magnitude requested by this proposal. Is such approach feasible? Yes, inasmuch as there really are not a lot of alternative approaches. An integrated measurement-modeling approach is far better than one of them alone. Where I see the project is most likely to generate novel information (and the part of the proposal that I found most intriguing), was the characterization of the spatial variation through the "sensor web network" using sensor pods. Water resources processes in complex terrain (especially in the Sierra Nevada) is the norm, not the exception, and we need new approaches to better understand this spatial, and resulting temporal, distribution. I see the incorporation of this web network the most interesting and unique aspect of this study. Throwing lots of instruments in one geographic region will answer questions if money is no object, but I feel that science should go beyond such approaches. My overall rating in this section is "Very Good", based on an "Excellent" for the attention paid to the spatial variation issues through the use of sensor pods, but a "Good" for the otherwise standard approach, not supported by specific details and justifications.</p>
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	<p>My greatest concern with this proposal was that the approach was not fully documented. I do not feel that this will limit the likelihood of success, however, based on the track record of the PI and associated Co-PIs. For example, relying on fluxes measurements from one tower (I appreciate the cost involved with tall towers, hence why more than one was not a</p>
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	<p>budgetary option), should require a discussion of flux footprint models that would be used to quantify the sampling area, hence the placement of the "meteorological towers" within this footprint. Details on how these sub-canopy meteorological measurements would be "extended outwards" were not given (not a trivial undertaking). Details on how the challenges of turbulent flux measurements in complex terrain would be dealt with were missing. Important details on how the sensor pod data would be scaled to the tower flux measurements were lacking. Details on how each of the many hypotheses will be tested were not given; rather a general "smorgasbord" of methodologies were presented. That said, I believe that meeting the goals and objectives of this proposal are "within the grasps" of the authors. They can do all of this, they simply didn't give the details in the proposal due to length limitations, I assume.</p>
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre-post comparisons; treatment-control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	<p>"Monitoring" is not really an appropriate criteria, as defined above (i.e. there are no treatments). Whereas it is implicitly mentioned that near real-time data will be made available (one for "raw-data"; one for derived products) via web sites, it is implied that there are plans to interpret long-term data sets. I may have missed it, but I didn't see any mention of timetables/schedules associated with each of the five specific tasks. Stating that each task spans months 1-36 is too vague (see "Tasks" section in the proposal). Information development within the context of the SNHO is an added bonus.</p>
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Rating	very good
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Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The integration of several observation strategies and platforms, the integration of this study with other agencies (e.g. USGS) and projects (e.g. SNHO), and the basic knowledge of the spatial variability and controls of snow melt in the Sierra Nevada derived from this study from capable PIs ensure that products of value will be obtained from this study. Links to larger management systems were clearly mentioned. Attention to data dissemination was provided, and the track records of the PIs leave little doubt that interpretable outcomes will be provided. The carbon balance aspect, however, is superfluous and not developed enough or pertinent to the main research and CALFED objects to be considered a research product.
Rating	excellent

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	A non-issue.
Rating	excellent

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Budget

Is the budget reasonable and adequate for the work proposed?

Comments	<p>The budget is high, over a million dollars for a three-year study. It is appreciated that field studies are expensive, but this budget is still on the high side and more representative of studies involving the use of aircraft. It appears the inflated budget results from equipment costs in Tasks 1 and 2, representing 56% of the budget (see my table below). The costs for the flux tower are justified and reasonable (the NR-Lite radiometer does not cost \$2000 (more like \$1000, unless they went way up in price)), however, there is no justification for the \$200K sensor web, other than saying JPL is the only provider. For such a large budget item and important aspect of the proposal, details on this should have been provided. A significant portion of the budgets is for salaries and benefits, with hourly rates as high as \$95.60. The salary and associated benefits costs represent 32% of the total budget (\$370K/\$1,142K).</p> <p>Task Cost (in thousands of dollars) % of Total Budget (1,142K)</p> <table><tr><td>1</td><td>Flux Tower</td><td>236</td><td>21</td></tr><tr><td>2</td><td>Ground Sensor Network</td><td>396</td><td>35</td></tr><tr><td>3</td><td>Remote Sensing</td><td>156</td><td>14</td></tr><tr><td>4</td><td>Water Balance Modeling</td><td>203</td><td>18</td></tr><tr><td>5</td><td>Disseminate Results</td><td>151</td><td>13</td></tr><tr><td></td><td>Salaries/Benefits: all tasks</td><td>370</td><td>32</td></tr></table>	1	Flux Tower	236	21	2	Ground Sensor Network	396	35	3	Remote Sensing	156	14	4	Water Balance Modeling	203	18	5	Disseminate Results	151	13		Salaries/Benefits: all tasks	370	32
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	Salaries/Benefits: all tasks	370	32																						
Rating	good																								

Overall

Provide a brief explanation of your summary rating.

Comments	<p>My summary rating for each category is summarized in the table below. I calculated my overall rating as: $5+5+4+3+4+5+5+3 = 34/40$, equivalent to 4.25/5.00 (based on "Excellent" = 5... "Poor" = 1), which rounds to a "Very Good" on your scale.</p>
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	<p>Category Rating Summary Comment Goals E Well developed, clearly stated and supported.</p> <p>Justification E The case for the justification of this study is strong.</p> <p>Approach VG Standard "scale from point-to-grid" coupled measurement/modeling approach taken. Integration of sensor prods unique, but not well explained.</p> <p>Feasibility G Details describing how spatial measurements will be integrated, and how the hypotheses presented will be tested were not well developed.</p> <p>Monitoring VG Here too, details were too vague. Timetable for all tasks was 1-36 months.</p> <p>Products E No problems, based on the track records of the PIs.</p> <p>Capabilities E No problems. Fully capable.</p> <p>Budget G Suffers from lack of details in Task 2, and salaries/benefits representing 1/3 of the greater than \$1 millon 3-year budget.</p>
Rating	very good

Technical Review #2

proposal title: A measurement strategy to monitor and assess changes in the Sierra Nevada water balance using satellite and ground observations

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	GOALS
	<p>Clearly Stated? The "Project Purpose" (pp. 1-8) provides a comprehensive rationale for the proposal. That section clearly makes the case for improved observation (surface- and remotely-based), spatial and temporal scale extrapolation, and data integration in understanding the snow-drive hydrologic regime of high-elevation mountainous regions of western North America, including California. Review would have been facilitated by a concise statement of specific project objectives up-front; not until p. 9 are the four Project Goals and Objectives presented. That said, those goals are appropriate to both the Sierra Nevada and broader water management issues facing mountainous sectors of western North America. They are also goals which have been broadly recognized over the past three+ decades (in my personal experience), and toward which researchers in the US and Canada (and Fennoscandia and Europe) have been working since the 1960's.</p> <p>Seasonal snow drives the hydrologic cycle at high elevations; snow input, redistribution, and partitioning to sublimation, melt, evapotranspiration, soil moisture recharge, and streamflow are critical to water availability for ecosystem processes and for downstream application to social/economic needs. The</p>

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	<p>goals to better measure, understand, model, and at some point "manage" these processes in complex, heterogenous mountain landscapes are entirely appropriate.</p> <p>Internally consistent? Yes</p> <p>Timely? Yes - growing recognition of the role of snow in water resources, coupled with increasingly variable seasonal snow deposition and ablation at high elevations (possibly in part related to climate change or global warming), and with burgeoning human population and growing demands for water, makes this topic very timely.</p> <p>Important? Yes - as stated above.</p> <p>Rating: 1</p>
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	JUSTIFICATION
	<p>Justified relative to existing knowledge? Yes - the investigators have pretty well defined their proposed research in relation to current scientific knowledge of spatially distributed mountain snowpack dynamics. The justification in terms of CalFed water management needs is less clearly stated and seems almost to be taken as a "given." In my personal view, the research IS fully justified in view of the increasing demands for increasingly sophisticated hydrologic process models for both water availability forecasting and for</p>

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	<p>more efficient management of water yielded from snowmelt and rainfall.</p> <p>Conceptual model clearly stated? Yes - the linkage of in situ ground-based and satellite-based measurements, applied to improved runoff estimation and utilizing spatially distributed process models, is clearly laid out. (It should be noted that this is not a new or unique concept - but the investigators seem very well positioned to implement the concept in this setting.)</p> <p>Explains basis for proposed work? Yes - the basis is clearly explained in the first eight pages.</p> <p>Selection of project justified? Yes - the project is presented as a prototype or field demonstration project to validate this approach to more comprehensive, basin or watershed-scale near-real-time hydrologic data acquisition and application. While it is clearly a first step in the PI's goal of a NSF-funded Sierra Nevada Hydrologic Observatory, this proposal itself is not contingent of their possible success in the NSFO competition. If funded, this project could stand alone and make a significant contribution to snow hydrology and water management in the central Sierra Nevada.</p> <p>Rating: 1</p>
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be

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useful to decision makers?

Comments	APPROACH
	<p>Well designed? Generally, yes - the linkage of an intensively instrumented flux tower site and associated "measurement cluster" and satellite-based measurements tied to remotely sensed, spatially distributed landscape-scale data is a widely applied concept and appropriate to this project. The increased field measurement sophistication afforded by the "distributed sensor web" taking advantage of new wireless technologies is an appropriate advance in field application, and if successful will be a welcome augmentation of already-available field measurement techniques.</p> <p>There is some concern about the proposed design. The investigators rightly note the bias of most available snowpack (and weather and climate) measurements toward lower-elevation, easier-access locations, and the limitations of standard NRCS Snow Survey and SNOTEL sites (which by design are intended to support developing long-term regression relationships for streamflow forecasting, rather than necessarily being "representative" of larger landscape units). The PI's stress (for instance on page 12) the importance of site selection - "Site selection along Tioga Road is of primary importance....; ...selecting the most representative locations for observations is critical" and "The basin-wide distribution of physiographic variables for the Merced and Tuolumne River basins will guide the site selection for the spatial distribution of the sensor networks along Tioga Pass Road." However, it appears that the availability of Crane Flat and existence of the 60-m (or is it 120-m, 60 m to each side?) corridor along the road, technically not in wilderness status, governs actual instrumentation site availability. Since both Crane Flat and Gin Flat are valley locations, they cannot be reasonably considered as "representative" of the</p>

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larger landscape. The narrow non-wilderness road corridor traverses a more topographically diverse landscape , and apparently they presume (see discussion on p.16) that they can locate appropriate sites for the spatially distributed sensor network within that corridor. Will such sites be sufficiently far from the road to eliminate or minimize influence of the road itself - the cleared roadway and right-of-way, altered vegetation within the right-of-way, effects of road maintenance and traffic, and modified local topography? And, since the selected site(s) are necessarily within the road corridor, will it/they really be "representative" of the larger landscape as the proposal suggests? The restrictions and advantages (logistical, political) of working within Yosemite are acknowledged - it would be helpful to have this aspect more clearly explained.

In the discussion of sensor pods and sensor web modules (p 12-13, one statement caught my attention - "...the sensor web allows for a distributed, embedded sensing presence that can also react and adapt to changing environmental conditions." The sensors are installed at fixed locations with fixed measurement points - so how do the sensors "adapt to changing conditions"? (Yes, I do understand the potential for the distributed sensor web to at least partially overcome the problem of single-point measurements.)

Again on p. 13, with regard to snowpack measurements it is stated that "Manual depth surveys will be done..." I suggest that traditional depth and SWE measurement, rather than depth alone, would be appropriate. Yes, snowpack mean density is less variable than snowpack depth on any given sampling date, but it takes but a little more time to pull and weigh the sampler (I don't mean to dig snow pits at each visit!) and thus acquire more comprehensive information on local snowpack variation.

Throughout the proposal (for instance, top of page 14)

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there is reference to "stream stage measurements," and the proposal suggests adding a stage measurement station on Moss Creek. This may be just a terminology quibble, but stream stage alone is of limited utility; I must assume that they mean stage measurement in a control section with development of a stage-discharge relationship for measurement of stream discharge, the variable of interest. The third paragraph on p. 14 mentions "Crossbow" technology; that technology is not defined, apparently is not to be incorporated into the current proposal, and the reference seems irrelevant to this proposal other than allowing them to reference yet another aspect of their wide-ranging research program.

It is unclear to me how the investigators plan to actually incorporate Winstral et al's work on snow redistribution by wind - was that intended to assist with analysis of data from the sensor web site(s), or with basin-wide gridded snowpack data?

Appropriate for meeting objectives? Yes - feasibility of the flux towers has been widely demonstrated at multiple locations; application of remote sensing products to snow hydrology has been widely investigated, as very briefly covered in the proposal; the distributed sensor network and wireless technology has less history but should work in the relatively benign warm-snowpack environment of the Sierra Nevada; utilization of Leavesley's widely-adopted PRMS/MMS modeling system is appropriate both for the research and for "downstream" application of project results to CalFed needs in water management.

Feasible? Yes, the field instrumentation, remote sensing data acquisition, data processing and modeling are very "doable" given the funding potentially available.

Likely to add to base of knowledge? Yes

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	<p>Likely to generate novel information/methodology/approaches? Yes - the integration and results dissemination of Tasks 4 and 5 will generate "novel" near-real-time information and data sets.</p> <p>Information ultimately useful for decision makers? Yes - the near-real-time data sets and hydrologic modeling products should be valuable to operational hydrologists and policy levels within state and feral agencies.</p> <p>Rating: 2</p>
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	<p>FEASIBILITY</p> <p>Approach fully documented and technically feasible? Yes, the approach is adequately documented, and technically feasible as discussed above.</p> <p>Likelihood of success? Given the experience of the PI and colleagues, I would suggest that there is high probability of success. This kind of research is more often set back by un-anticipated events - wipeout of instruments by avalanche or flood or meddling tourists (or in my case, inquisitive bears), failure of a satellite transmitter - than by lack of skill or planning. I think they would pull it off.</p> <p>Scale consistent with objectives and within the grasp of authors? Yes - within the larger landscape, they are really proposing a limited-scale pilot instrumentation/data acquisition/data processing/modeling effort. The ultimate goal of the</p>
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	SNHO with stations distributed along elevational and latitudinal gradients within California is also appropriate - and this specific proposal would be a good first step in coordination with Goulden's installation further south. Rating: 2
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre-post comparisons; treatment-control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	MONITORING Monitoring appropriately designed? Yes but reference my concerns about "representative" site selection, above. Plans to interpret monitoring data? Yes - as laid out in Goals 3 and 4, and Tasks 4 and 5. In terms of operational water management, I suggest that Task 5 could more clearly spell out how the information and data will be integrated into operational snowmelt and water supply forecasting, and regulation of downstream sectors of the system, in the near term - who will directly benefit and how? Rating: 2
Rating	very good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

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Comments	<p>PRODUCTS</p> <p>Products of value likely? Yes - but, repeating my comment above, I suggest that Task 5 could more clearly spell out how the information and data will be integrated into operational snowmelt and water supply forecasting, and regulation of downstream sectors of the system, in the near term - who will directly benefit and how?</p> <p>Contributions to larger data management systems relevant? Interpretive or interpretable outcomes likely? Yes, utilizing the PRMS/MMS modeling outputs - but, again, it is not clearly defined how the research products will get into the hands of the operational water folks, and how it will be incorporated into day-to-day management systems.</p> <p>Rating: 3</p>
Rating	good

Additional Comments

Comments	<p>ADDITIONAL COMMENTS</p> <p>Knowing the long-term scientific contributions which have come from the Central Sierra Snow Lab at Truckee, I was a bit surprised to see no reference to possible incorporation of that site (as a more northern satellite location), or to the monumental early snow hydrology work accomplished there, anywhere in this proposal.</p> <p>Another quibble - the investigators repeatedly refer to actions or data sets that will "compliment" their proposal, or which their proposal will "compliment" - for instance, in the Executive Summary, on p. 1, and at the top of page 20. I don't question that they are</p>
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	<p>polite - but "compliment" means "a courteous act" or "to say something in praise or courtesy." "Complement" means to supply a lack or to make more complete - and is the correct term. Spell-check won't catch it!</p> <p>And another - in the second paragraph, p. 16, there is reference to "closed-cone coniferous forest" as one of three main cover types. I am not familiar with either a closed-cone forest or an open-cone forest. Conifer cones open for seed dispersal; might they have meant to refer to closed-canopy stands?</p> <p>And another - the caption for Figure 4 (p. 5) indicates "Statistical distribution of snow depth measurements...surrounding 4 SNOTEL sites..." but there are six bar graphs in the figure.</p>
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	CAPABILITIES
	<p>Authors' track record? Excellent. Bales is widely recognized as competent and productive, as indicated by his publication record. The other PI's have fully appropriate qualifications and productivity, commensurate with their shorter careers to date.</p> <p>Qualified? Yes - so near as I can judge, they are all full qualified for their respective tasks and responsibilities in this project.</p> <p>Available infrastructure and support? Yes - but this is a little harder to judge. It appears that the support needed for the flux tower and the distributed embedded sensor network is available within their respective institutions. NPS support in terms of permitting and facilitating access to the research site seems assured. It is not so clear that there is</p>

Technical Review #2

	full support for the final task, making the data stream and PRMS/MMS modeling outputs available to and useful to the water managers. Rating: 1
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	BUDGET Reasonable and adequate? Yes - for a three-year proposal of this scope, this is a reasonable budget. Rating: 2
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	<p>The proposed research project is timely, is appropriate to present and emerging water resources needs in the Sierra Nevada (and throughout the seasonal-snow-dominated (for water supply) regions of western North America).</p> <p>The proposed site location has many advantages -- scientific, political and logistical -- but some limitations in terms of finding appropriately "representative" instrumentation sites within constraints of access and land use designations (wilderness).</p> <p>The research team has an excellent publication record; the PI is closely linked with important regional and</p>
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#0239: A measurement strategy to monitor and assess changes in the Sierra Nev...

Technical Review #2

	<p>national hydrologic science initiatives and planning efforts; the technical expertise of the team members is excellent.</p> <p>There is every reason to have strong confidence that the investigators will accomplish the instrumentation, data processing and hydrologic modeling outlined in the proposal.</p> <p>Actual linkage of the research program with operational hydrology and water management by state and federal agencies is not clearly delineated in the proposal.</p>
Rating	very good

Technical Review #3

proposal title: A measurement strategy to monitor and assess changes in the Sierra Nevada water balance using satellite and ground observations

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The proposal is very well written and provides an fairly complete description of the context and timeliness of the proposed research.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	<p>The science aspect of the work is justifiable. I am not convinced that the PIs have invested time and effort to systematically assess and quantify the relative value and weaknesses of the existing observational networks. Clearly, in a science context, a case for more data can be made to support modeling etc.</p> <p>One suggestion to the PIs would be to pursue observational network design by carrying out modeling studies that selectively examine the sub-watershed scale sensitivity of presence/absence of observations of the variety planned as part of this project, as well as, including/dropping existing point observation from model assimilation and understanding processes</p>
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#0239: A measurement strategy to monitor and assess changes in the Sierra Nev...

Technical Review #3

	and target variables that are most sensitive to such observational deficiencies. Short of that, I feel a bit concerned about an applications research program, such as, CALFED supporting this research, which may not have deliverables can that decisively support decision-making.
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approach is reasonable. Understanding and modeling the uncertainties in remotely sensed data is a real open issue, the proposed work would do well to devote significant effort in this direction. Higher resolution modeling may not improve streamflow forecasting, if the uncertainties are not adequately understood and modeled.
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The research ideas and implemented look quite reasonable.
Rating	very good

Technical Review #3

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	very good
Rating	very good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	New observations in near-realtime are excellent products. Data access, reliability and management may be of import to maximize data use for decision support.
Rating	very good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	excellent
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

#0239: A measurement strategy to monitor and assess changes in the Sierra Nev...

Technical Review #3

Comments	very good
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	This is a very good research proposal. Some comments/concerns are noted above. Testbeds of this variety will be important engines that drive CALFED science and research into the future.
Rating	very good